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ALIVE AND KICKING: A REVIEW OF HANDBOOK OF BEHAVIORISM, *EDITED BY WILLIAM O'DONOHUE AND RICHARD KITCHENER*

WILLIAM M. BAUM

UNIVERSITY OF NEW HAMPSHIRE

Behaviorists have struggled and continue to struggle with basic questions about behavior, such as how to define behavior, how to talk about behavior in relation to environment, and what constitutes an adequate explanation of behavior. Skinner made huge progress on these questions, because of his emphasis on the generic character of stimuli and responses, his advocacy of rate as a datum, his introduction of stimulus control, and his reliance on selection by consequences as a mode of explanation. By no means, however, did he provide final answers. In particular, Skinner fell short because he never escaped from the limitations imposed by thinking in terms of contiguity and discrete events and because he never specified a useful role for theory. The 14 chapters in this book offer varying degrees of clarity on the ways in which behaviorists and behaviorally oriented philosophers dealt with basic questions in the past and are dealing with them in the present, post-Skinner. They are reviewed individually, because they are uneven in quality. Overall, the book is a useful tool for gaining historical and philosophical background to behaviorism and for getting some idea of behaviorists' current directions.

DESCRIPTORS: behaviorism, history of behaviorism, philosophical behaviorism, psychological behaviorism

Should behavior analysts be interested in behaviorism? Those who say no regard philosophy as unimportant to science. Those who say yes believe that theory partly determines method, both in experiments and in practical settings. Those who think there can be no method without theory and that theory is likely to be implicit in methods also probably believe in making theories explicit. At the least, knowing the assumptions underlying one's methods may avoid embarrassment in social encounters with philosophers, professional or amateur.

Contrary to the claims of its antagonists, behaviorism is alive and well. The evidence is that behavior analysis is alive and well, and behaviorism is the philosophy underlying behavior analysis. Behaviorism neither died nor stopped developing with Skinner. Although Skinner's contributions were great and made a science of behavior possible, a number of fundamental issues remain to vex behaviorists for the foreseeable future.

Here, for example, are three: (a) What exactly is behavior? More precisely, what sorts of terms should we use to describe behavior? (b) What terms should we use to talk about relations between behavior and environment? (c) What is an adequate explanation of behavior? Although the three questions are interrelated, each has its special poignancy.

On the question of how to conceive of behavior, Skinner made progress in two ways. First, he pointed to the generic nature of behavior, both respondent and operant. Reinforcement strengthens, not some specific muscle movements, but a whole class of movements. Second, he rejected the goal of predicting the moment of an operant's occurrence in favor of predicting the frequency of its occurrence. With these two innovations, Skinner freed the science to measure

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I dedicate this review to the memory of Ullin Place, who was a good friend to behaviorists everywhere.

Correspondence with William Baum should be addressed to 611 Mason Street, #504, San Francisco, California 94108 (E-mail: wm.baum@unh.edu.)

behavior by its effects and by its rate, facilitating a flood of new types of experiment and treatment. He stopped short, however, of dealing with behavior in relation to time. His treatment of responses as discrete events left us with the problem of how to deal with extended activities such as building a house, keeping sober, or being in love. Skinner failed to appreciate that momentary behavioral events have no meaning; they are classified only in the context of what went before and what came after. A particular lever press might be operant or accident; we judge from other occurrences before and after it.

On relations between environment and behavior, Skinner made great progress with the concept of stimulus control. This liberated behavior analysts from having to think in terms of reflexes and stimulus–response bonds, allowing us to think instead about behavior occurring in context. The versatility of Skinner's idea of stimulus control, however, clashed with the narrowness of his concept of reinforcement, which relied solely on contiguity between discrete response and discrete consequence. What are the consequences of building a house, of keeping sober, of being in love? A concept of reinforcement, primary or secondary, that focuses only on moments in time needs to be replaced with something more plausible.

On the question of what should be accepted as an explanation, which philosophers group with the question of how theories relate to evidence, Skinner helped by defining and criticizing mentalism. Behaviorists now tend to be good at exposing spurious explanations of behavior, including the "physiologizing" of the cognitive psychologists. Skinner was vague, however, about what sort of theories would be acceptable. He emphasized environment and history, pointed to the power of selection by consequences, and suggested that theory might be driven by data. However, most of his writings about theory were negative in tone, and he never developed any theory of his own about behavior. (Unless you count the reflex reserve, which he had to retract.) As behavior analysis has become more quantitative, it has also become more theoretical, and thorny questions arise. For example, other sciences include hypothetical concepts like atoms and genes. When are hypothetical concepts acceptable in explaining behavior?

Given this state of affairs, this new book, *Handbook of Behaviorism,* edited by O'Donohue and Kitchener, offers a welcome tool for understanding the history, philosophy, and current status of behaviorism. It consists of a collection of 14 chapters, which I roughly categorized in a two-by-two table: current versus history or background on one side and science versus philosophy on the other. I will say something about each one, because the chapters are notably uneven in quality.

The introductory chapter by O'Donohue and Kitchener, for example, offers little of the guidance one might have wished for. Instead of attempting to define the common elements that make a view behavioristic, which doubtless would have taken a lot of thought and perhaps some courage, they instead assert that behaviorism might be a word that should occur only in the plural; there are many behaviorisms. The editors did, however, cast a wide net. The result is that just about any major thinker who ever called himself or was called by others a behaviorist is represented.

The book includes chapters on Wittgenstein, Ryle, and Quine (classified as background, philosophy) and Skinner's philosophy, contextualism, and logical behaviorism (classified as current, philosophy). These complement and contrast with chapters more directly related to the science of behavior: on Watson, interbehaviorism (Kantor), Hull, Tolman, and Bijou (classified as background, science); and on teleological behaviorism (Rachlin), theoretical behaviorism (Staddon), and biological be-

haviorism (Timberlake), which I classified as current, science.

More times than I care to remember, I have gone through a cycle with respect to philosophy. First, philosophical writing catches my interest, and I begin reading. After a time, the fascination gives way to frustration, and I wind up dropping the pursuit. The philosophers seem to be asking really good questions about how to think about life and behavior, but they never seem to get anywhere. Doubtless that conclusion is what makes me a scientist; I want to see practical, empirical results. When the philosophy chapters in the book are contrasted with the science chapters, a difference appears. The philosophers are interested in logic, absolute truth, and fixed language meanings, whereas the scientists are interested in empirical study, inductive truth, and creation of meaning.

The reason that a handbook like this should include both psychological behaviorism and philosophical behaviorism is that the two share a kinship. What defines behaviorism may be found in the writings of the early behaviorists, such as Watson, Hull, and Skinner. All of them held to the proposition that a natural science of behavior is possible. I suggested in my own book, *Understanding Behaviorism* (Baum, 1994), that that proposition defines behaviorism. I think it applies also to the philosophers who are considered behaviorists; it is just implicit rather than explicit. A philosopher would be classed as a behaviorist if he or she would answer affirmatively to the question, "Is a natural science of behavior (apart from mind) possible?" I believe Quine, Ryle, and possibly Wittgenstein would, even if grudgingly or disinterestedly, answer yes.

The best chapters in the book are among those that I classified as current. Howard Rachlin's chapter on the view he calls teleological behaviorism represents progress on all three of the questions I raised earlier. Starting from Skinner's ideas that behavior consists of movements of the whole organism and must be defined in terms of context and consequences, Rachlin adds that behavior consists of patterns of movements and consequences that are more or less extended in time. In one fell swoop, he extends the conception of behavior, incorporates the environment into the conception, and clarifies the types of theory that could be valid. The phrase "more or less" applied to temporal extension carries a lot of weight, because Rachlin makes use of Aristotle's categorization of causes, arguing that the full understanding of behavior requires two kinds of theory: one in terms of efficient causes and one in terms of final causes. Patterns are more or less extended in time because more extended patterns include less extended ones. In this sense, descriptions of more extended patterns are more "abstract" than descriptions of less extended patterns. As it takes less time to observe one lever press than to observe the fixed-interval (FI) performance into which it fits, so it takes less time to see someone driving a nail than to see that person building a house. Descriptions like "pressing the lever," "making the FI scallop," "driving a nail," and "building a house" define behavior in terms of its effects or "goals." Hence the name *teleological.* Rachlin calls the kind of theory that fits less extended patterns into more extended patterns a theory of final causes. It contrasts with theories of efficient causes, what people today usually mean by the word *cause*: an immediate determinant. Most people, including philosophers, prefer efficient causes. That prejudice drives cognitive psychologists to talk about inner causes, and Rachlin offers an alternative, not only to mental causes but also to private events such as thoughts, sensations, and perceptions. The one problem with Rachlin's presentation is that his use of teleology and final causes will arouse unnecessary skepticism. He could instead have talked about history and mechanism or,

even better, used the terms *proximate cause* and *ultimate cause* from evolutionary biology. Unfortunately, he makes no contact with evolutionary theory and the more general concept of selection by consequences, which would be both a natural ally and an accepted mode of explanation.

If Rachlin overlooks selection by consequences, the excellent chapter by Jon Ringen, "Radical Behaviorism: B. F. Skinner's Philosophy of Science," makes up for the omission. It contains a careful and lucid explanation of the connection between Skinner's conception of operant behavior as behavior selected by consequences and his criticisms of mentalism. Although Skinner's objections to "theories" are often, incorrectly I believe, thought to constitute the basis of his objections to mentalism, Ringen makes it clear that the derivation goes the other way around: His skepticism about "theories" derived from his rejection of mentalism. Ringen correctly sees that Skinner's objections to mentalism, in turn, derived from his recognition that selection by consequences offers a scientifically acceptable alternative. The difficulty of talking about apparently intelligent behavior without introducing the noun *intelligence,* Ringen compares to "thinking" "impossible" "thoughts." As it seemed in the 19th century impossible to think about design in nature without a designer, so it seems impossible to the critics of radical behaviorism to think about intelligent behavior without intelligence. That its critics consider radical behaviorism to be "incoherent," Ringen answers by suggesting, "the incoherence is no more than a failure of imagination, rather like that embodied in the nineteenth-century charge of incoherence leveled at those who suggested there could be design in nature without a designer of nature" (p. 175). Unfortunately, no other chapter emphasizes the centrality of selection by consequences or its relation to evolutionary theory.

The chapter by John Staddon, "Theoretical Behaviorism," aims only to counteract what Staddon sees as behaviorists', particularly Skinner's, unnecessary antipathy toward theory. He seeks to prevent any impression that he might return to mentalism by first offering a nice brief critique of cognitive psychology on the grounds that it is mentalistic. After this, the main part of the chapter consists of an argument in favor of theories of behavior incorporating "states." It makes an interesting contrast with Rachlin's chapter. Whereas Rachlin suggests that a full understanding of behavior requires specifying both final causes and efficient causes, Staddon remarks that it is "better to find efficient causes—the antecedent factors that determine behavior and the mechanisms through which they act—if we can" (p. 224). He means that we need efficient causes for prediction and control. Few would disagree with that point, but he uses it to argue in favor of states as hypothetical efficient causes. He is struggling with two problems: the importance of history and the impossibility of defining behavior in terms that are strictly physical. To address the problem that past events have effects in the present, he proposes that past events produce hypothetical states in the present. He offers as an example the possibility of training 2 pigeons to peck at the right key of two adjacent keys, but 1 of the pigeons having first been trained to peck at the left key; the behavior of the 2 pigeons is identical, but their subsequent behavior during extinction will be very different. I was reminded of Skinner's (1932/1968) argument against "learning" and "conditioning" as inner states, in which he compared 2 rats, neither of which is pressing a lever, but 1 of which has been trained and satiated and the other never trained. As Rachlin would say, the difference between the 2 organisms cannot be determined at the moment, but any hidden "residue" really only consists of "more behavior." Both Skin-

ner and Rachlin would question the need for an inner state to explain the difference. For the problem of defining behavior, Staddon suggests that *doing* should be defined in a theoretical model that offers an account of the mechanism of the behavior. Rachlin's conception of *doing*, in contrast, consists of defining behavior in terms of temporally extended patterns. In the end, Staddon never clearly distinguishes his ideas of states and models from cognitivist theories and appears dangerously close to imagining immediate causes that may impede rather than aid understanding. On the need for theory, particularly in a quantitative science, however, one can hardly disagree.

Disappointing for its lack of appreciation of selection by consequences, given its title, is William Timberlake's chapter, "Biological Behaviorism." Instead of providing an up-to-date integration of behaviorism with evolutionary concepts, the chapter merely revisits some "biological" limitations discussed in the 1970s (e.g., adjunctive behavior) and offers an undeveloped proposal of theory construction from an animal-centered view. Timberlake appears to echo Staddon's call for understanding mechanism by the construction of hypothetical models. He prefers "causal" models that are animal centered, but I never understood what he meant by this. He proposes to take the animal's point of view to make initial guesses about efficient causes in the environment, and then to test hypotheses developed from those guesses. I saw nothing new, only an erroneous connection of "traditional" behaviorism with logical positivism and advocacy for Tolmanian intervening variables and Hull's hypothetico-deductive method. I thought the chapter was too long and too full of unexplicated prose, like "biological behaviorism is not the science of external behavior. It is a science of the dynamics of living forms in the external realm" (p. 275). How the "dynamics of living forms" differs from behavior and what distinguishes the "external realm" were never clarified.

The two remaining chapters that I classified as current are both philosophically oriented. Kitchener, in his chapter called "Logical Behaviorism," illustrates well the goals of philosophical treatments of behaviorism. He lists at least eight different varieties of behaviorism, probably none of which, except methodological behaviorism, would be familiar to a behavior analyst. He proceeds to distinguish between analytic behaviorism and logical behaviorism, both of which he regards as varieties of semantic behaviorism, his name for the notion that mentalistic terms bear a logical or semantic connection to behavior. Analytic and logical behaviorism both hold that "every mentalistic term M refers to (means) a set of behaviors B and/or behavioral dispositions BD" (p. 401). For analytic behaviorism, however, the behaviors and behavioral dispositions are "criteria for the (correct) application of M," whereas for logical behaviorism, they are "the verification basis (evidence) for the application of M." A behavior analyst might wonder what the force of such a distinction could be; it turns out that the concept of "verification" is tied to many other philosophical concepts about true statements, syntactics, and evidence. Kitchener goes on to exploit inconsistencies and vagueness in the writings of logical positivists (mostly Carnap and the little-known Neurath) to argue that elements of psychological behaviorism (mostly naturalistic epistemology) may be found lurking there, even if on most readings the logical positivists seem to be at odds with contemporary behaviorism. A behavior analyst interested in science is left wondering how this attempt could be useful; it seems useful only in a philosophical enterprise aimed at logical coherence and absolute truth.

I also classified as current, philosophical the chapter by Elizabeth Gifford and Steven Hayes, "Functional Contextualism: A Prag-

matic Philosophy for Behavioral Science." My reaction to this chapter was mixed. It contains a useful discussion of mechanism in contrast with pragmatism. I enjoyed the attempt to distinguish functional contextualism, which the authors view as a type of pragmatism, from descriptive contextualism, which is often seen as antithetical to science. A possible weakness, however, lies in their failure to couple their emphasis on practical goals with an explanation about how goals are chosen. Their ideas apply to science in general, not just behavior analysis. This perhaps explains the superficiality of their treatment of concepts crucial to behavior analysis, particularly the definition of behavior as "act in context" and the temporal extension of behavior. They make the same points as Rachlin, only much less well. At times, I found the writing unintelligible: "purpose involves the past as the future in the present" (p. 296). Their criticisms of Skinner also seem off the mark; they point to relatively trivial inconsistencies in his uses of stimulus and response, when the problems lie with his reliance on discrete events and contiguity. Their claim that private events may be studied scientifically includes no treatment of the problem of privacy, with the result that it sounds mysterious. Although they use the phrase repeatedly, they never explain how one would conduct the "analysis of private events."

Among the chapters that I classified as background, several are excellent. The chapter on Watson, by Edward Morris and James Todd, is the best brief treatment of Watson's contributions I have seen. It is thorough and, best of all, addresses the ambiguities in Watson's writings. The authors distinguish Watson's metaphysical behaviorism from methodological behaviorism, they discuss Watson's shifts with respect to the roles of instinct and environment, and they point out the variation in Watson's use of "prediction and control" in different contexts.

The chapter on Hull, by Michael Rashotte and Abram Amsel, gives a good picture of his thinking and that of Spence, who followed. The authors are partial; they appraise Hull's ideas in the most positive light possible, which is probably good, because his ideas have failed the test of time. I recall in my student days, when Hull was required reading, how I loathed his use of intervening variables, all those aggravating symbols, and his theory spinning in the absence of data. In retrospect, I think that Hull was handicapped by a mechanistic approach to behavior relying solely on stimulus and response as momentary events. Probably one could argue that Staddon's theoretical behaviorism is the modern successor, with the difference that today we have data against which to test hypothetical models. If you want a clear appreciation of Hull, this chapter will serve.

I also thought the chapter on Tolman, by Nancy Innis, was thorough and fair. She makes a sympathetic appraisal, while recognizing that Tolman's ideas also have failed the test of time. His "purposive" behaviorism might be compared with Rachlin's teleological behaviorism, because both stress goals or ends. The two differ radically, however, because Rachlin treats purpose and motive as more behavior, whereas Tolman put them inside the organism. His internal intervening variables resulted in an embarrassing dualism that he never surmounted. He rarely suggested how the inner constructs (''needs,'' ''tensions,'' ''hypotheses,'' etc.) were to be measured, except to suggest, sounding like a methodological behaviorist, that behavior such as vicarious trial and error (a supposed vacillation by a rat at a choice point in a maze) might by taken as "objective correlate(s)" (p. 106). To her credit, Innis faces the question of whether Tolman ought to be considered a behaviorist at all. She thinks he should be, but I found it hard to agree. To me, he appears to fit the pattern of most cognitive psychologists, who combine methodological behaviorism with a definition of psychology as the science of inner

processes. If you read her chapter, you may judge for yourself.

The brief chapter by Sidney Bijou is really an autobiographical memoir. He explains how he sampled various thinkers, but finally settled on Kantor and Skinner. It may be interesting as a primary source for historians.

I tried hard to understand the chapter on Kantor and interbehaviorism by Linda Hayes and Debra Fredericks, but I found it baffling. My experience with it resembled my other encounters with interbehaviorism. I began reading with interest, because the chapter promised to contend with central issues such as defining behavior and environment. Then suddenly I found myself in an impenetrable jungle of undefined jargon. Early on, the authors follow the word "interbehaving" with "[i.e., interacting]." When I tried substituting interact for interbehave and interaction for interbehavior, I got no further ahead. The authors use a plethora of unfamiliar terms (matrix, field, transcendence, organismic, etc.) without explanation. Here is a sample sentence, discussing interbehavioral fields: "The events making up such fields [according to Kantor], consisted of adjustments of organisms to environing things, evolved from bioecological interbehaviors" (p. 81). I was unable to discover how events can make up fields, how adjustments evolve, or what "bioecological interbehaviors" might be. To their credit, the authors try to compare Kantor's interbehaviorism with Skinner's radical behaviorism. But the same lack of clarity prevailed; much of what they said about Skinner sounded incorrect (''What evolves throughout the course of ontogenetic history in Skinner's formulation is an organism, not functional relations, as held in interbehaviorism," p. 88), and I came away with the impression that interbehaviorism represents no advance over radical behaviorism.

In contrast, the three background chapters on philosophers with some relation to behaviorism—Wittgenstein, Ryle, and Quine—are all excellent and useful. The chapter by David Bloor on Wittgenstein, whose writings are notoriously difficult to interpret, presents a remarkably clear explanation of Wittgenstein's views on language and privacy, particularly of his argument against a private language. I found it delightful because of the parallels to Skinner. Bloor, however, argues that Wittgenstein may be seen as a behaviorist only by selective reading. He focuses on the difference between avowals of pain and introspective reports of sensations (e.g., blue color). Avowals of pain, even though they might arise from a private sensation, receive a social warrant that introspective reports do not. In Bloor's view, Wittgenstein was a collectivist, whereas Skinner was an individualist. I think this may be a misreading of Skinner, because it fails to take account of Skinner's (1957) emphasis on the verbal community. *Verbal Behavior* was omitted from the references. (Unfortunately, so was Willard Day's 1969 paper on Skinner and Wittgenstein.) Whether Bloor's characterization of Wittgenstein is right or not, this is a really good chapter for sharpening one's appreciation of the philosophical issues involved in radical behaviorism.

I also enjoyed Ullin Place's chapter on Ryle, who was a colleague of Wittgenstein's and made many related points about language and mentalism. The chapter is technical and probably requires one to have read Ryle's (1949) *The Concept of Mind.* Place assesses both Ryle's achievements and his critics. I disagreed with his dismissal of Ryle's criticism of dualism as the "ghost in the machine." At the end, however, I was convinced that despite his achievements in analyzing mental terms as behavioral dispositions, Ryle failed to deal adequately with what Skinner called private events and covert behavior, events or processes "taking place beneath the individual's skin to which he or she has some kind of 'privileged access' that is not available to another person" (p. 380).

Place goes on to argue, however, that for Ryle, as for Skinner, this residue of terms is small and relatively unimportant for understanding most behavior.

Finally, Roger Gibson's chapter on Quine was excellent. Quine appears to be the philosopher who most accepted behaviorism. As Gibson presents it, Quine's approach to language closely resembles Skinner's ideas about verbal behavior. He insisted repeatedly that even if one might entertain mentalism in other contexts, in talking about language, "the behaviorist approach is mandatory" (p. 431). His treatment of mentalistic terms resembles that of Ryle and Rachlin; he identifies them with behavioral dispositions. An important difference, however, is that Quine equates behavioral dispositions with physiology, "states of nerves." Gibson brings out Quine's awareness of the weaknesses of such an identity theory of mind (e.g., its openness to abuse and its insufficiency), but never suggests that Quine overcame these. Quine's writings might allow behaviorists to bridge the chasm that separates psychological behaviorism from philosophical behaviorism. This chapter offers a good place to start.

In summary, *Handbook of Behaviorism* should be a useful tool for anyone who wishes to understand the key issues with which behaviorists have struggled. I recommend reading it selectively, because of the uneven quality of the chapters, but for historical background, for philosophical background, or for gaining some idea of present directions, the book should be a big help.

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